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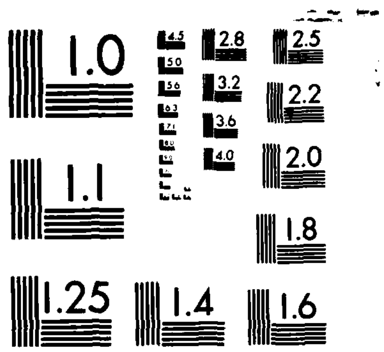

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IMPACT OF LINEAR PROGRAMMING ON COMPUTER DEVELOPMENT

by

George B. Dantzig

TECHNICAL REPORT SOL 85-7

June 1985

Department of Operations Research
Stanford University
Stanford, CA 94305

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A-1

IMPACT OF LINEAR PROGRAMMING ON COMPUTER DEVELOPMENT

by George B. Dantzig[1]

During World War II, I was a branch chief under Tex Thornton, head of Statistical Control, United States Air Force (USAF), Pentagon. Thornton later founded Litton Industries. Some of us in Stat Control were or since have become quite famous: Brandon Barringer, Philadelphia banker; Ed Learned of Harvard Business School; Warren Hirsch, mathematician, now at N.Y.U.; Arjay Miller, Ben Mills, and Ed Lundy who became President, Vice President, and Treasurer, respectively, of Ford Motor Company; and Robert McNamara of Secretary of Defense and the World Bank.

My office, Combat Analysis, developed statistical factors, such as sortie rates, needed for Air Force planning. I trained members of the Air Staff on how to compute Air Force deployment programs by hand and was considered an expert on practical <u>programming</u> methods -- the origin, incidently, of the term Linear Programming. For these efforts, I received four special citations.

Most important, for what I am about to relate, was my awareness of the work going on at the Bureau of Labor Statistics by my friends Duane Evans, Jerry Cornfield, and Marvin Hoffenberg on the Leontief Input-Output Model of the American Economy.

The postwar story begins in June 1946. After defending my Ph.D. thesis in Berkeley, I returned to the Pentagon -- I was still in the job

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market. J.D. Williams of Douglas Aircraft Company, Santa Monica, in an August 1946 letter to me suggested that there would seem to be definite mutual advantages to our establishing and maintaining some sort of liaison between the Combat Analysis Section of Headquarters AAF Statistical Control and the RAND project. He wanted to discuss with me such a possibility. Reading between the lines, liaison meant he was offering me a job. It took me six years to accept. A lot happened in those six years that you will soon see.

It all really began when Dal Hitchcock, an advisor to General Rawlings, the Air Comptroller, and Marshall Wood, an expert on military programming procedures, cooked up an elaborate plot to entice me to stay in the Pentagon. They believed they had a sufficiently challenging problem to keep me from looking for a position elsewhere. Wood proposed I develop some kind of analog device which would accept as input equations of all types and numbers and ground rules and use these to generate as output a consistent Air Force plan. This was in the fall of 1946 before we were aware of the possibility of an electronic digital computer.

My approach to Wood's proposal was to formalize the mathematical structure by looking for simplifying and unifying principles. Of course, I thought first to try to adapt the Leontief Input-Output Model. But Marshall and I also talked about certain hierarchical schemes that more or less aped the actual military planning process. Because the underlying matrix structure was triangular, this became known as the Triangular Model. Our research effort later became known as Air Force Project SCOOP (Scientific Computation of Optimal Programs).

In late fall 1946 we began to hear rumors about a marvelous new invention, the electronic computer. Wood and I went to Aberdeen Proving Ground in November to see if such computers could be used for computation of Air Force plans. Early in 1946 we attended what to us was a science fiction fantasy, a Symposium on Large-Scale Digital Calculating Machinery at the Harvard Computation Laboratory where Howard Aiken had already built the Mark I. We were impressed! Various "Buck Rogers" types spoke at the meeting about their dreams so vividly, so realistically, and with such conviction that we became true believers that electronic computers and artificial intelligence were almost, if not already, a reality!

After the conference Marshall and I visited a Boston pub where a jukebox played any tune requested by simply talking into a mouthpiece. "Look," we said to each other with great wonder in our eyes, "it is happening. Already in a pub there is a voice-recognition device coupled with an electronic gadget which selects any requested record and plays it for us." We were soon dissolutioned when a drunken sailor asked it to play Swannee River with a tongue so thick and words so slurred that no nonhuman could have possibly understood him. Nevertheless, from then on all our plans assumed that one day a digital electronic computer would exist that could do the calculations. Meanwhile, we would get ready by using punch-card equipment.

In February 1947 I wrote a letter to Tex Thornton, who at that time was still with the Ford Motor Company. I told him about my presentation to General Rawlings on the possibility of a "program integrator" for planning and scheduling. I commented how impressed I was with Ed Rawlings' strong backing of our project and how it appeared he would support us to the tune of a half-million dollars if required. I find the letter interesting because it helps me recall some of my early dreams. I quote, with minor changes: "I believe the central problem facing us in planning, whether for a large business like Ford, a grocery store, or for analysis of a war economy, involves the analysis of the interrelation of a multitude of components on some outcome. Generally, the interrelationship of one component with its neighbor is easily obtainable -- just like the position of a tree next to its immediate neighbors in the forest can be obtained, but it is difficult to integrate this information in order to find out something about the whole complex. Let us suppose that one could design, however, an electronic machine that in a sense simulates the relationships among components; suppose further that this machine can operate so fast and has such a vast memory for detail that it can take information from its memory, perform 1,000 calculations a second, and store or print the results; and, finally, suppose that these suppositions are not merely pipe dreams, but such developments as the Harvard Mark II, the ENIAC and EDVAC, and Massachusetts Institute of Technology (MIT) gadgets were well along this very road, then one could begin to see the possibilities of this new work."

Formulating the Model

In May 1947 my research began in earnest. The model developed was a generalization of Leontief's Input-Output System to make it dynamic, to have alternatives, and to have an objective function to drive the system.

If I were asked today who was the most important person to make all these ideas happen, I would say General Ed Rawlings. He had the money and the courage and imagination to allow us to use it. In June 1947 he approved a transfer of $400 thousand to the National Bureau of Standards (NBS) that initiated much of the mathematical research there under John Curtis, and electronic computer research also there under Sam Alexander. The Bureau used our funds, in turn, to support development of UNIVAC and IBM computers.

It indeed may be true that much of the postwar development of electronic computers can be traced to the direct and indirect sponsorship of the AAF Comptroller's Office. I know $400 thousand doesn't sound like much, but it was a lot of money in those days.

From the viewpoint of future development of linear programming (LP) theory, the most helpful man at NBS was Albert Cahn, who first pursuaded me to contact T.C. Koopmans and John von Neumann. After conferences in Philadelphia in mid-May 1947 with various contractors, including Eckert and Mauchley, and in Boston in June with MIT on computers, I took Kahn's advice and in June visited Koopmans.

T.C. Koopmans became the focal point and the spur of economists' interest in LP. He was instrumental in having Leo Hurwicz spend some time with me in Air Force Headquarters in August 1947.

Origins of the Simplex Method, Summer 1947

The first idea that would occur to anyone as a technique for solving a linear program, aside from the obvious one of moving through the interior of the convex set, is that of moving from one vertex to the next along edges of the polyhedral set. I discarded this idea immediately as impractical in higher dimensional spaces. It seemed intuitively obvious that there would be far too many vertices and edges to wander over in the general case for such a method to be efficient.

When Hurwicz came to visit me at the Pentagon in the summer of 1947, I told him how I had discarded this vertex-edge approach as intuitively inefficient for solving LP. I suggested instead that we study the problem in the geometry of columns rather than the usual one of the rows -- column geometry incidently was the one I had used in my Ph.D. thesis on the Neyman-Pearson Lemma. We dubbed the new method "climbing the bean pole". It looked to me efficient.

I felt sufficiently confident in this special case of what later became known as the simplex method that I proceeded to modify it so that it would work for linear programs without a convexifying row. I also developed a variant for getting a starting feasible solution called Phase I. It was then that I discovered that the method was really the previously discarded vertex-edge procedure in disguise (except for an added criterion for selecting the edge on which to move). Apparently, in one geometry the simplex method looks efficient while in another it appeared to be very inefficient! Thus the simplex method was born in August 1947.

First Meeting with von Neumann, Fall 1947

On October 1, 1947 I visited von Neumann for the first time at the School for Advanced Study at Princeton. I remember trying to describe to him, as I would to any ordinary mortal, the Air Force problem, beginning with the formulation of the linear programming model in terms of activities and items, and so forth. Von Neumann did something which I believe was uncharacteristic of him. "Get to the point," he said impatiently. Having a somewhat low kindling point myself at times, I said to myself, "Okay, if he wants a quick version of the problem, that's what he will get." In under one minute I slapped the geometric and the algebraic version of the problem on the blackboard. Von Neumann stood up and said, "Oh that!" Then for the next hour and a half, he proceeded to give me a lecture on the mathematical theory of linear programs.

At one point, seeing me sitting there with my eyes popping and my mouth open -- after all I had searched the literature and found nothing, von Neumann said: "I don't want you to think I am pulling all this out of my sleeve on the spur of the moment like a magician. I have just recently completed a book with Oscar Morgenstern on the theory of games. What I am doing is conjecturing that the two problems are equivalent. The theory that I am outlining for your problem is the analog of the one we have developed for games."

Thus I learned about Farkas' Lemma and about duality for the first time. Von Neumann promised to give my problem some thought and to contact me in a few weeks. He did write to me proposing an iterative scheme which Alan Hoffman and his group at the Bureau of Standards around 1952 compared with the simplex method, also with proposals of Motzkin and others. By the way, the simplex method in these 1952 tests came out a clear winner.

The First Test of the Simplex Method

Sometime during the fall of 1947, Marvin Hoffenberg of the Leontief Input-Output team of the Bureau of Labor Statistics suggested we should try the simplex method on the minimum cost diet problem. Margaret Reid of the Bureau of Home Economics in the Department of Agriculture was most cooperative in helping me find the data I wanted. When she finally understood exactly what my problem was, she icily suggested that I read the article by Stigler, "On the Cost of Subsistence". Naturally I was puzzled why Reid had become so uptight and most curious to read Stigler's paper. It was a marvelous paper and I couldn't understand why anyone would take exception to it until I read the footnote at the very end of the paper: "Tax-supported bureaucrats and professors may also have another reason for certain of their practices." Stigler's paper remains a classic to this day about the difficulties of formulating a model.

In December 1947 (or January 1948) Jack Laderman of NBS and his group of about 25 at Mathematical-Tables Project in New York City got busy solving Stigler's diet problem using desk calculators -- the first real test of the simplex method.

It was perhaps a year or so later, after many tests by my staff and by NBS using desk calculators, that we became convinced that the simplex method was really working and that it was better to bet on it than look further into other techniques. It is doubtful if any of the developments that followed, including the events at this meeting today, would have happened if the simplex method had not turned out to be efficient.

Influence of Albert W. Tucker, the Year 1948

The year 1948 was the year of assessment, polishing, directly and indirectly subsidizing groups outside the Air Force, and the organization of SCOOP. The year began with the first statement of the duality theorem and the use of the term "dual". In my unpublished paper dated January 5, 1948 titled "A Theorem on Linear Inequalities" is the following conclusion:

> **... therefore that if there exists any solution to (41) there exists a solution to the dual system (43) and in particular there exists a simultaneous solution of the form (46) to (50) and because of (51)**
>
> **(52)** $$\text{MAX } x = \text{MIN } w$$

Thus the duality theorem was born. The paper is my proof of a theorem conjectured to me by J. von Neumann during my visit with him in Princeton in October 1947.

This was a time of many exciting discussions with Murray Geisler and Bob Dorfman of the Air Force Operations Research Group. Bob Dorfman, I quote him, used to "beat a path" to my door. Murray Geisler joined SCOOP as a branch chief.

There were trips to Princeton by Koopmans and myself to see von Neumann. We discussed the merits of several numerical methods for solving matrix-minimax problems. I conferred with Leontief at Harvard about my generalization of his approach, which I called <u>Programming in a Linear Structure</u>. I visited with Professor Learned of the Harvard Business School. John Curtis took me to Princeton to meet his brother-in-law, Albert W. Tucker, head of the Mathematics Department.

Yes, 1948 was a year of great ferment and change. The name of the field was changed. It happened this way: On June 28, 1948 Tjalling Koopmans and I took a walk along the beach in Santa Monica. He said "Programming in a Linear Structure" was much too long. Why don't I shorten it to "Linear Programming". I liked his idea and said: "From now on that is what the field will be called."

At the end of July, John Curtis held an important symposium on computers and numerical methods at the University of California, Los Angeles. Speakers included John von Neumann, who spoke on "Electronic Methods of Computation"; D.R. Hartree, Cambridge University on "General Survey of Current British Developments" (re computing machinery) and "Some Unsolved Problems in Numerical Analysis"; D.H. Lehmer, University of California, Berkeley, on "Numerical Methods in Pure Mathematics I"; Hans Rademacher, University of Pennsylvania, on "Numerical Methods in Pure Mathematics II"; Bernard Friedman, New York University, on "Wave Propagation in Hydrodynamics and Electrodynamics"; Solomon Lefschetz, Princeton, on "Numerical Calculations in Non-Linear Mechanics"; Herman H. Goldstine, Princeton, "Eigenvalues and Eigenvectors for Symmetric Matrices"; and I spoke on "Programming in a Linear Structure", the old name of the field. Others present were Howard Aiken of Harvard and Ida Rhodes of the National Bureau of Standards.

Project SCOOP

Project SCOOP (Scientific Computation of Optimal Programs) officially started during this period. In August 1948 Marshall Wood and I briefed the Air Staff on the use of electronic computers in military planning. We stated, I quote:

1. "The primary objective of Project SCOOP is the development of an advanced design for an integrated and comprehensive system for the planning and control of all Air Force activities."

2. "The recent development of high-speed digital electronic computers presages an extensive application of mathematics to large-scale management

of problems of the quantitative type. Project SCOOP is designed to prepare the Air Force to take maximum advantage of these developments."

I tried to explain what a computer does. See Exhibit 1 at the end of this paper.

I paraphrase: It shows the inputs, the outputs, and the contents of the external library of the USAF program computer, the electronic computer, and the high-speed printer. Fourteen different large-scale digital computers are being developed or have been developed up to this time, mostly with the support of the Army and the Navy. See Exhibit 2. The three computers supported by the Air Force are the NBS Interim recently negotiated with the Bureau, which it is hoped would be ready in 10 to 12 months for calculations of small programs; the UNIVAC for the Comptroller and ERA for Wright Field which is some 18 months away. There is also NBS *SUPERSPEED that exists only as a possible project* for the fiscal year 1950 budget.

Up to that time only one electronic computer had been built, the ENIAC at Aberdeen. There were other computers in operation, but these were electromechanical relay machines similar in principle to IBM punch-card equipment. A feature of many of the machines in the design phase or under construction at the time was the use of an acoustic delay line, made of liquid mercury, as the internal memory of the computer.

The computers listed in Exhibit 2 show a wide variety of speeds. The relay machines were not significantly faster than existing IBM punch-card equipment. At the other extreme, the "superspeed" computers -- still in such a preliminary stage that they were little more than a gleam in one's eye -- were being designed to perform at the rate of 50 thousand multiplications per second; the machines actually under construction at the time were expected to operate on the order of 1 thousand per second.

In the fall of 1948 there was great excitement about game theory. At the famous Madison, Wisconsin joint meeting of the American Mathematical Society, the Institute of Mathematical Statistics, and the Econometric Society, von Neumann chaired the session on the theory of games, where I presented the simplex method in the geometry of the columns as "climbing the bean pole". It looked efficient.

I also presented von Neumann's "center of gravity" method and an extension of it which I now call "Von Neumann's least squares". Philip Wolfe has since developed it and published "Finding Nearest Point in Polytope", Math Prog 1976. It takes more work per iteration but seems to take fewer iterations than phase 1 of the simplex method. After my talk the chairman called for discussion. There was a moment of silence; then a hand raised -- it was Hotelling's. I must hasten to explain that Hotelling was huge. He used to love to swim in the ocean and when he did it is said that the level of the ocean rose perceptibly. Hotelling stood up in the back of the room. His face was expressive. It took on one of those all-knowing smiles we all know so well. He said devastatingly, "But we all know the world is nonlinear." Then he majestically sat down. And there I was, a virtual unknown, frantically trying to compose the proper reply to the great Hotelling.

Suddenly another hand in the audience was raised. It was von Neumann. He was to have been the chairman for the session but he had decided to sit with the audience and let someone else do the honors. Mr. Chairman, Mr. Chairman," he said, "if the speaker does not mind, I would like to reply for him." Naturally I agreed. Von Neumann continued: "The speaker titled his talk 'Linear Programming' and carefully stated his axioms. If you have an application that satisfies the axioms, then use it. If yours doesn't, then <u>don't</u>," and he sat down. In the final analysis, of course, Hotelling was right. The world is highly nonlinear. Fortunately systems of linear inequalities (as opposed to equalities) permit us to approximate most of the kinds of nonlinear relations encountered in practical planning.

Transportation Models, the Year 1949

In January 1949 the National Bureau of Standards, under the supervision of Franz Alt, solved the first large-scale 27 x 64 Hitchcock-Koopmans transportation problem using the simplex method. Betty Boch, economist with the Federal Trade Commission at that the time, had been analyzing how much costlier were steel products because the steel industry computed transportation as if the steel was being shipped from a <u>basing point</u> instead of the actual factory where it was produced. To assist her with her analysis, she proposed a comparison between the actual cost of transportation under the basing-point policy to what would have been the cost if the shipment to customers from steel mills had been done by solving the minimum cost transportation problem. The comparative costs for the homogeneous product we selected turned out to be $794 thousand versus $556 thousand.

The solution, making use of a tree in a graph, took about 9 man days of hand calculation. For reasons obscure to me, I did not describe the use of a tree in my paper on the transportation problem in Koopman's "Activity Analysis". A graph of the optimal tree can be found in my original notes. It had been prepared for publication with the article but was never used. You will note in Exhibit 3 that the tree has 27 nodes corresponding to the 27 origins instead of the usual 27 + 64 destination nodes.

Solving Triangular Model Using Card Calculations

In early 1949 Michael Montalbano of NBS developed a clever system of using IBM punch-card equipment to solve Air Force triangular models. Sorters, 602s, collators, reproducers, and 407 tabulators were arranged in a circle to facilitate high-speed processing of machine cards. IBM equipment was very unreliable in those days. Multiple machines were used for verifying that each step was correctly computed. The 602s were wired to carry out some 50 programmed steps. This was necessary because the Air Force Triangular Model was quite sophisticated with complicated detailed steps to be performed.

In the spring of 1949 the first electronic computer code was developed -- also for the triangular model. I did the coding myself for the BINAC computer being built at the time for Northorp by Eckert and Mauchley. Also in the spring of 1949 Project SCOOP began to implement some of its research and it was necessary to organize ourselves on several fronts. This shift in focus was expressed by two basic documents. In the first, "Prospectus for an A/F Mathematical Computation Center", I proposed a plan for practical computation. The other was entitled, "The Need for

High-Speed Electronic Computers for Programming", which I later presented to the RDB Committee of Basic Physical Sciences (August 1949).

In a June 1949 memorandum I noted that two auxiliary devices were also required before full utilization of a computer could be possible. First were magnetic tapes on which data was stored prior to their introduction into a computer. Tape preparation would be a Herculean job in itself because the number of coefficients found in an Air Force problem ran into hundreds of thousands and perhaps as high as tens of millions. The most suitable way to get the data onto tapes appeared to first punch holes into IBM cards and then use special machines to convert the information on cards onto magnetic tapes. Second was a high-speed printer that would get the results from the computer out into a form where they could be read and distributed to the Air Staff. Early in March 1949 additional funds were transferred to the Bureau of Standards for these two purposes, bringing funds transferred to date for all purposes slightly over a half-million dollars.

Koopman's Activity Analysis of Production and Allocation (published in 1951) was the proceedings of a symposium on linear programming that took place in June 1949. The conference was the culmination of two historic years of intense research. How did so much happen in such a short period? In my opinion it was, first, the pent-up energy of people emerging from a war period. Second was the drive of expert planners like Marshall Wood and Murray Geisler who were raring to go. Third was an exciting new breed of economists: Koopmans, Samuelson, Arrow, Dorfman, Simon, and others. Then there were the mathematicians like von Neumann, Tucker, Kuhn, and Gale who saw a beautiful structure that could be used to describe competition and to control compexity. Finally there were entrepreneurs like General Rawlings who had the $$$$s. On the theoretical side, there were the two related but quite separate streams of effort -- game theory and mathematical programming -- that just happened to come together at just the right time in history. But that was not all. Just around the corner were the computers! Everything clicked.

Not that this period was without its traumas. There was a sad letter in December 1949 from John Curtis of NBS begging for a little money to help Harry Huskey's SWAC computer.

Six months later, June 20, 1950, the SEAC computer was dedicated, and in early September I gave a talk before the Association for Computing Machinery on how SEAC would be used to solve linear programs. In early October, bureaucracy began to take over. NBS decided that its function was to give advice on the design of computers for other government agencies.

How well did SCOOP do in fulfilling all the promises it made earlier? Here are some amusing excerpts from a memo dated May 8, 1951, from General Dau to General Rawlings, on this very subject.

> **I suppose there is some belief that we have not accomplished all we set out to do.... With respect to SCOOP, we are engaged in an educational project which will never end.... I am pleased the unwarranted publicity attached to electronic computers has diminished.... It now appears SEAC will not produce much more than education and experience.**

In June of 1951 the first symposium on mathematical programming was held in Washington, D.C. It should have been called the second for the first one was the famous conference of Koopmans back in Chicago in 1949. Since that date, every three years there has been a symposium in places like Washington, Santa Monica, Rand, London, Princeton, The Hague, Stanford, Budapest, Montreal, and Bonn. The twelfth such symposium is scheduled for August 1985 at MIT. Each one of these symposia has been highlighted by some important new breakthrough.

I now conclude my talk on the impact of linear programming on computer development. As of January 1952, six months before I left the Air Force and the Pentagon to go to Rand, the Air Force had invested over a million dollars with the National Bureau of Standards for development of computers and computing methods. This was a lot of money in those days to be spent on research.

USAF PROGRAM COMPUTER

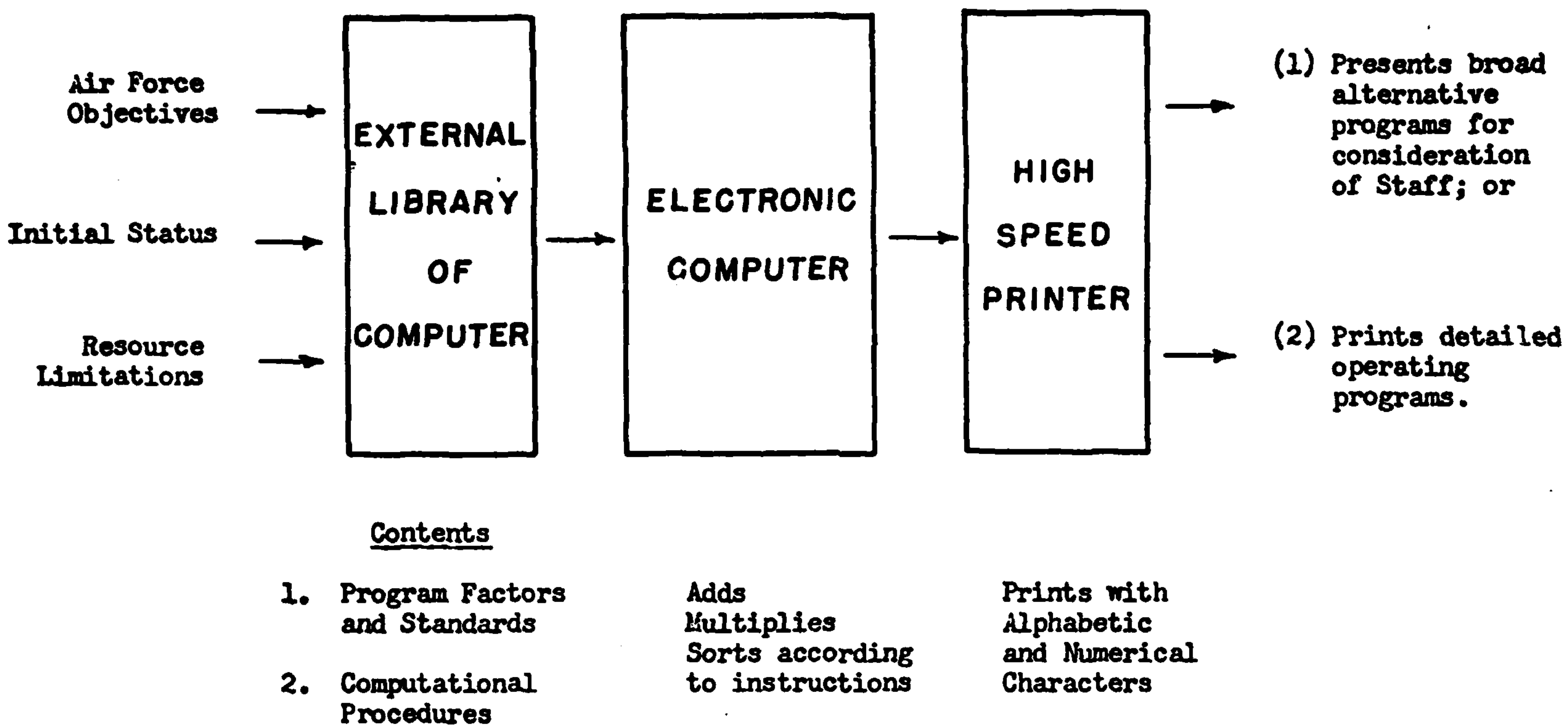

Exhibit 1

LARGE SCALE DIGITAL COMPUTERS

STATUS AS OF 1 JULY 1948

MACHINE	SPONSOR	BUILDER	LOCATION OR DESTINATION	INPUT AND OUTPUT		INTERNAL MEMORY			ARITHMETIC UNIT		STAGE OF COMPLETION
				MEDIUM	SPEED WORDS/SEC	TYPE	CAPACITY	TRANSFER TIME SECONDS	OPERATION	SPEED SECONDS	
1. BALLISTICS COMPUTER	NDRC BRL	BTL	Ft. Bliss (1) BRL (1)	Punched Paper tape	1.2	Relay	10 five decimal digits	.4	Addition Multiplication	2.0	In Operation
2. RELAY COMPUTER	BTL NACA BRL	BTL	NACA (1) BRL (1)	Punched paper tape	.33 .55	Relay	30 seven decimal digits	.07	Addition Multiplication	0.3 1.0	In Operation
3. MARK I	Navy	IBM Harvard U.	Harvard U.	Punched cards or paper tape	.40 .40	Mechanical counters	3.72 twenty-three decimal digits	.3	Addition Multiplication	0.3 3.0	In Operation
4. MARK II	Navy Bu. Ord.	Harvard U.	Dahlgren Bu. Ord.	Paper tape	.66	Relay	100 ten decimal digits	.033	Addition Multiplication	.2 .7	In Operation
5. MARK III	Navy Bu. Ord.	Harvard U.	Dahlgren Bu. Ord.	Paper or Magnetic tape	13.	Magnetic drum	4000 sixteen decimal digits	.004	Addition Multiplication	.00066 .00600	Construction
6. SSEC	AAS Columbia U. IBM	IBM	New York IBM	Punched cards or paper tape	25. 20.	Relay Electronic	150 nineteen and 8 nineteen decimal digits	.07 and .0002	Addition Multiplication	.001 .020	In Operation
7. ENIAC	Army Ordnance	U. of Penn. Moore School	Aberdeen BRL	Punched cards	13. 10.	Electronic counters	20 ten decimal digits	.0002	Addition Multiplication	.0002 .0028	In Operation
8. EDVAC	Army Ordnance	U. of Penn. Moore School	Aberdeen BRL	Paper tape Magnetic wire	.30 28.50	Acoustic	1024 twelve decimal digits	.0002	Addition Multiplication	.00005 .002	Service Test
9. UNIVAC	Bu. Stand. Census USAF	Eckert Mauchly Corp.	Washington Pentagon Census	Magnetic tape	833. 833.	Acoustic	1000 eleven decimal digits	.000175	Addition Multiplication	.00100 .00224	Construction
10. RAYTHEON	Bu. Stand. ONR	Raytheon Mfg.Co.	Princeton Inst. of Numerical Analysis	Magnetic tape	450. 450.	Acoustic	1000 ten decimal digits	.000197	Addition Multiplication	.00073 .00090	Design
11. IAS	IAS RCA Army Ord.	Inst. for Advanced Study	Princeton IAS	Magnetic wire	1000.	Electrostatic	4096 eleven decimal digits	.000025	Addition Multiplication	.000050 .000100	Design
12. NBS Interim	NBS and USAF	NBS	Los Angeles	Magnetic tape	350.	Acoustic	500 ten decimal digits	.0002	Addition Multiplication	.0001 .0003	Construction
13. ERA	NBS and ONR	Engineering Research Associates	Washington ONR	Paper tape	20.	Magnetic drum	16,000 ten decimal digits	.008	Addition Multiplication	.016 .016	Design Study
14. NBS SUPERSPEED	USAF	NBS	Washington USAF	Magnetic tape	12000.	Acoustic	Undecided	Undecided	Addition Multiplication	.000020 .000020	Future Project

Exhibit 2

MINIMUM BASIC TONNAGE MATRIX:

Total Cost:	$556,154
Original Cost:	$794,253
Total Saving:	$238,099

Tree Diagram

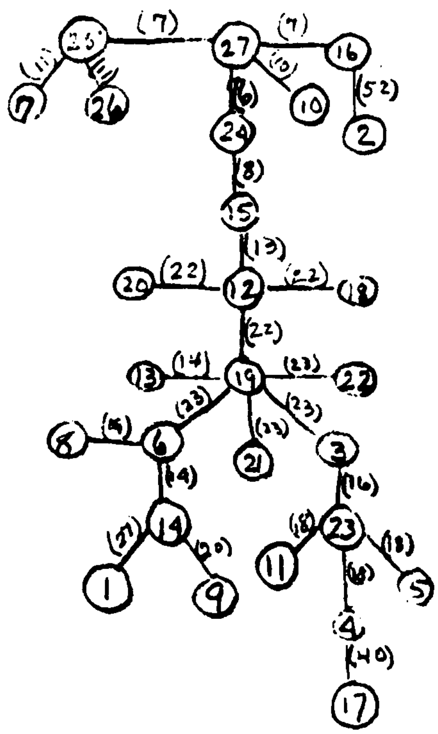

Exhibit 3

UNCLASSIFIED

SECURITY CLASSIFICATION OF THIS PAGE (When Data Entered)

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17. DISTRIBUTION STATEMENT (of the abstract entered in Block 20, if different from Report)

18. SUPPLEMENTARY NOTES

19. KEY WORDS (Continue on reverse side if necessary and identify by block number)

electronic computer development
linear programming
USAF Project SCOOP
simplex method
duality theorem
triangular models

20. ABSTRACT (Continue on reverse side if necessary and identify by block number)

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OL 85-7: Impact of Linear Programming on Computer Development, by George B. Dantzig

n the post-War period 1947 - 1952, the United States Air Force transferred onsiderable funds to the National Bureau of Standards to subsidize the early evelopment of computers, and peripherals such as high speed tapes and rinters. These funds, over a million dollars, help finance SEAC, SWAC NIVAC and early research of IBM.

his paper presents an intimate view of the network of colleagues of the uthor during this early period, their contributions, their sponsors, and why omputers were needed for the solution of a broad range of planning roblems. The story of how the simplex method was discovered is told once again.

[illegible] duality Theorem, [illegible] [illegible]

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